PROGRESS IN AVIATION.

APART from the unsuitability of the epithet, such an incident as is suggested by a recent *Daily Mail* poster, "Aëroplane Triumph; Expresses Collide; Passengers Injured," may not occur for some years to come, but the rapid development of aëroplane locomotion within the last year indicates that it may be desirable, at no distant date, to formulate "rules of the road" for aëroplanes crossing each other's path at the same level, and, unless this is done fairly soon, there may be the same difficulty in obtaining international uniformity that has always existed in such other connections as those of language, coinage, and measurement of time.

The records which have been published from day to day in the Press have now, for the first time, placed the problem of flight on a perfectly practical basis, and a great deal that has been written previously to the present year will only be read with interest now insomuch as it enables a comparison to be made between anticipation and realisation. We heard reports of the Wright Brothers' achievements in America in 1904 and 1905, but owing to the inventors' efforts to avoid publicity the feat of Santos Dumont on November 12, 1906, in covering 220 metres in 21·2 seconds has been regarded by many people as the first realisation of an artificially propelled man-carrying machine lifting itself from the ground and performing an actual flight.

M. Delagrange's aëroplane made flights of 164 feet and 196 feet in April, 1907, and by this time the construction of aëroplane machines began to be taken up from the commercial point of view by several firms in France. The present writer visited Captain Ferber in July of that year, and was shown a large building on the outskirts of Paris specially fitted up with the view of manufacturing aëroplanes to order, one being in process of construction. All previous authenticated records were eclipsed by Mr. Farman's flights in November of last year. Yet these records seem small by comparison with recent French achievements. In these, M. Delagrange figures prominently, as is shown by the following examples, selected without any claims to completeness:—March 21, Farman, 4·5 km. in 3m. 29s.; April 11, Delagrange (Archdeacon Cup), 3·925 km. in 6m. 30s.; June 22, at Milan, Delagrange, 15 km. in 16m. 30s.; June 23, at Rome, Delagrange, 17·5 km. in 18½m., touching ground once; July 6, Farman (Armengaud Prize), 20·4 km. in 20m. 20s.; September 6, Delagrange, 29m. 53·8s.

The Wright Brothers' performances take us a long way back, and include the following statements of flights:—September, 1905,[1] 17·961 km. in 18m. 9s., 19·570 km. in 19m. 55s.; October, 1905, 24·535 km. in 25m. 5s., 33·456 km. in 33m. 17s., 38·956 km. in 38m. 3s., the causes of stopping being exhaustion of fuel or hot bearings; May, 1908,[2] flights from 22s. up to 7m. 20s., with one man, distance 5 miles, two-man flights,[3] 0·45 mile in 29s., and 2·5 miles in 3m. 40s.; September 6, 1908, at Paris (Wilbur Wright), flight of 19m. 48s., and in America, September 12 (Orville Wright), flight of 74m. 20s. (these last on the authority of the daily Press).

The first "two-man" flight (in Europe, at any rate) would appear to date from March 21, when, after the flight recorded above, Mr. Farman mounted with M. Delagrange on the latter's aërodrome, which flew a considerable distance with the heavy load.[4]

[1] *American Magazine of Aëronautics*, July, 1907.
[2] *American Aëronautics*, June, 1908, quoted in *Aëronautics*, supplement to *Knowledge*.
[3] Photographs are given in the *Scientific American* for May 30, 1908.
[4] *Aëronautics (Knowledge)*, April, 1908.

This record, it will be observed, is earlier than the Wright records above chronicled. On May 30 Mr. Farman flew 1·241 km., with Mr. Archdeacon as a passenger, on his aëroplane.[1] Finally, we have a flight of more than 1½ hours by Mr. Wilbur Wright in France, shortly after the accident to his brother's machine in America, through which Lieut. Selfridge lost his life.

Simultaneously with these aëroplane experiments we have a series of chronicles of successes with the Zeppelin and other airships. We need only refer to the Zeppelin record of 11h. 50m., and a record by Major Gross of 13h. 2m., covering a distance of 187 miles.

A very interesting summary of progress in aviation up to the day of publication is afforded by M. Armengaud, junior's, book.[2] It is based on a lecture delivered on February 16 at the Conservatoire des Arts et Métiers, and it contains, in addition to an account of recent work, references to the early researches of Penaud, Marey, and others on flight of birds. A feature of special interest is the diagram showing the various systems of aëroplanes used by different experimenters. The illustration accompanying this article is based on the diagram in question, but we have omitted the purely gliding machines of Wright and Archdeacon, and have inserted the Farman "flying fish" type, as well as a figure of the mechanically-propelled Wright model based on the sketch in the *Scientific American*.

On looking at this table the typical Englishman whose education on current topics does not extend beyond the level of the halfpenny paper will ask, "Which is the best flying machine?" As the interrogator usually is under the prevalent delusion that "a straightforward answer to a straightforward question" is all that is necessary to settle, once and for all, the most complex problem of science, and as he probably will forget all that has been told him when he reads about the next football match, the best way of satisfying him is to give a definite answer by choosing one at random from this diagram and saying it is the best. A general discussion of the different types of flying machine, including, not only aëroplanes, but orthopters and helicopters, is given by M. Armengaud, and this probably contains as much as could be embodied in a small handbook. But a complete examination of the conditions required to give the best results involves the discussion of at least two qualities, efficiency and stability, and while engineers have shown themselves fully competent to deal with the first of these qualities, a full discussion of the latter still involves the expenditure of a large number of brain-power hours of work at the hands of a really competent mathematician, and it will be one of the objects of this note to direct attention to some of the most important unanswered questions involved in the theory sketched out some years ago by the present writer, with the assistance of Mr. Williams.

Captain Paul Renard's two papers on dirigibles[3] may at this stage be studied with advantage. The first paper is mainly theoretical, the second descriptive.

Taking the second part first, it contains an illustrated description of all the principal dirigibles that have been constructed, and of a number that have been projected. Captain Renard expresses the opinion that France, which has produced a Montgolfier and a

[1] *Ibid.*
[2] "Le Problème de l'Aviation, sa Solution par l'Aéroplane" (Paris : Ch. Delagrave.) Price 2.50 francs.
[3] "Les Aérostats dirigeables" (*Revue générale des Sciences*, June 15 and 30, 1908).

Col. Renard (whose early experiences with *La France* were greatly in advance of their time), is still to the forefront in aërial navigation by means of airships. He considers, further, that in spite of its interesting details of construction, the Zeppelin aërostat is not to be regarded as a model to be copied. These views we quote, without comment, on the authority of their exponent. The first or theoretical part contains a simple exposition of the elementary principles on which the success or failure of directed aërostats depends. In the first place, the relative velocity of propulsion (*vitesse propre*) must exceed the velocity of the wind if the aërostat is to be completely under control, otherwise the course will be confined within a limited angle. This fact every student of elementary mechanics ought to realise at a glance, but many who succeed in passing examinations fail to do so, and thus Captain Renard's remarks are not so superfluous as they might seem to be to a person who really understood elementary mathematics. As the present writer pointed out, it is mainly the difference in speed between air currents and ocean currents which has rendered aërial navigation less successful hitherto than ocean navigation.[1]

Captain Renard discusses the questions of permanence of form and the relative advantages of large and small screws, and then proceeds to the question of stability. He distinguishes three different kinds of stability, namely, stability in altitude, stability of course, and longitudinal stability. According to the conditions assumed in text-books, when a balloon is in equilibrium at any altitude that equilibrium is stable, so that "instability in altitude" is not a mechanical effect, but consists in the effects of physical causes in disturbing the vertical equilibrium of a balloon; in a dirigible there are many easy methods of maintaining a constant altitude. Instability of course or instability in a horizontal plane occurs when an aërostat tends to turn about a vertical axis so as to set itself at right angles to the direction of motion, like the ellipsoids of our text-books in hydrodynamics. In longitudinal instability the aërostat tends to turn about a horizontal axis, pitching over forwards or backwards. Captain Renard points out (and this is entirely in accordance with the present writer's investigations) that there is a certain limiting or *critical* velocity consistent with stability; in the case of the dirigible the critical velocity is a superior limit, which cannot be exceeded without the motion becoming unstable. He also clearly shows that this fact was known to Col. Renard in 1904, and further that the critical velocity in question in many types of machine, such as the *La France, Lebaudy,* and *Patrie,* has fallen considerably below the maximum speed obtainable from suitable motors. For example, "In the *Santos Dumont* the critical velocity is 8·50 m. (per sec.), and a 7 horse-power engine is sufficient to obtain it; if longitudinal stability were assured, the aërostat could be provided with a 22-horse-power engine and attain a speed of 12·10 m. For the *Lebaudy* the critical velocity is 10·80 m. requiring 41 horse-power. If this aërostat were stable it could carry a machine of 95 horse-power, which would give it a proper velocity of 14·20 m."

Yet we find another writer attempting to compare the stability of the *Patrie* and *Zeppelin* in a paper bristling with unnecessary mathematical formulæ, which do not even correctly represent the oscillations of the balloons about a statical state of equilibrium.[2] All that the calculation really does is to treat the balloons as simple pendulums the points of support of which are at the centres of buoyancy, and the masses

of which are concentrated in the cars. The use of the word "moment of inertia" tends to conceal the fact that the moment of inertia of the framework about its centre of gravity is completely ignored.

Passing on to the equilibrium and stability of aëroplane systems, we find that not only is there a widespread neglect of even some of the most elementary mathematical principles underlying the subject, but the experimental evidence commonly accessible is insufficient to enable any very definite conclusions to be drawn as to the best form of a flying-machine or as to how far the types which have admittedly given successful results are capable of improvement. The lift and drift of aëroplanes have been carefully measured, and so far as the problem of flight depends on their numerical magnitudes, the theory of the aëroplane is summed up "in a nutshell" on pp. 40, 41 of M. Armengaud's paper.

In the construction of motors the main, if not the only, object to be aimed at is to make the weight as small as possible for a given horse-power, a problem with which engineers have shown themselves sufficiently competent to deal. The best system of aëroplanes from the point of view of general efficiency is that which requires the least horse-power to sustain a given total load in horizontal flight. The actual arrangement of the planes will not affect the efficiency except when one plane is placed in the wake of another. But in connection with equilibrium and stability the conditions are very complex, and a great deal of difficult mathematics is required.

Take the question of propellers. The present critic makes no claim to have examined the literature that has collected around this problem in connection with its more or less closely allied applications to naval architecture, but it is certain that what has been found out regarding the efficiency of a ship's screw should form a starting point for discussions relating to airship propellers, account being taken of necessary modifications. Yet the most crude methods are forming the subject of published papers at the present time. C. M. Woodward's problems[1] would make suitable examples for a conventional text-book on "Dogmatics" (as dynamics should be called) if their working were correct, but the expression for the rate of working in driving an airship "thru" the air involves an error closely resembling that made when the oar is treated as a lever of the second class. The succeeding results regarding the horse-power applied to the "scru" would therefore be incorrect even if the fundamental assumptions were justified. W. B. Parsons[2] deals mainly with experiments, but it may be reasonably doubted whether he has really kept the *power* of his motor constant when the inclination of his blades has been varied. To do so the torque would have to be inversely proportional to the angular velocity. Neither the stated method of regulating the power nor the statement "The consequent variation in velocity is the expression of the air resistance for that inclination and velocity" (whatever this may mean?) appear reconcilable with this assumption.

But to come to the important question of stability, of which longitudinal stability, being the most important, shall alone be considered here. A large proportion of the contributors to aëronautical journals have the vaguest possible ideas as to what stability means. The successful flights of Farman, Delagrange, and Wright do not enable us to infer without further evidence that their machines are automatically stable. The analogous problem of the bicycle illustrates this fact. The lateral stability of the bicycle, like the

[1] *Cornhill Magazine*, May, 1907.
[2] Capt. Guido Castagneris in the *Aëronautical Journal* for July, 1908.
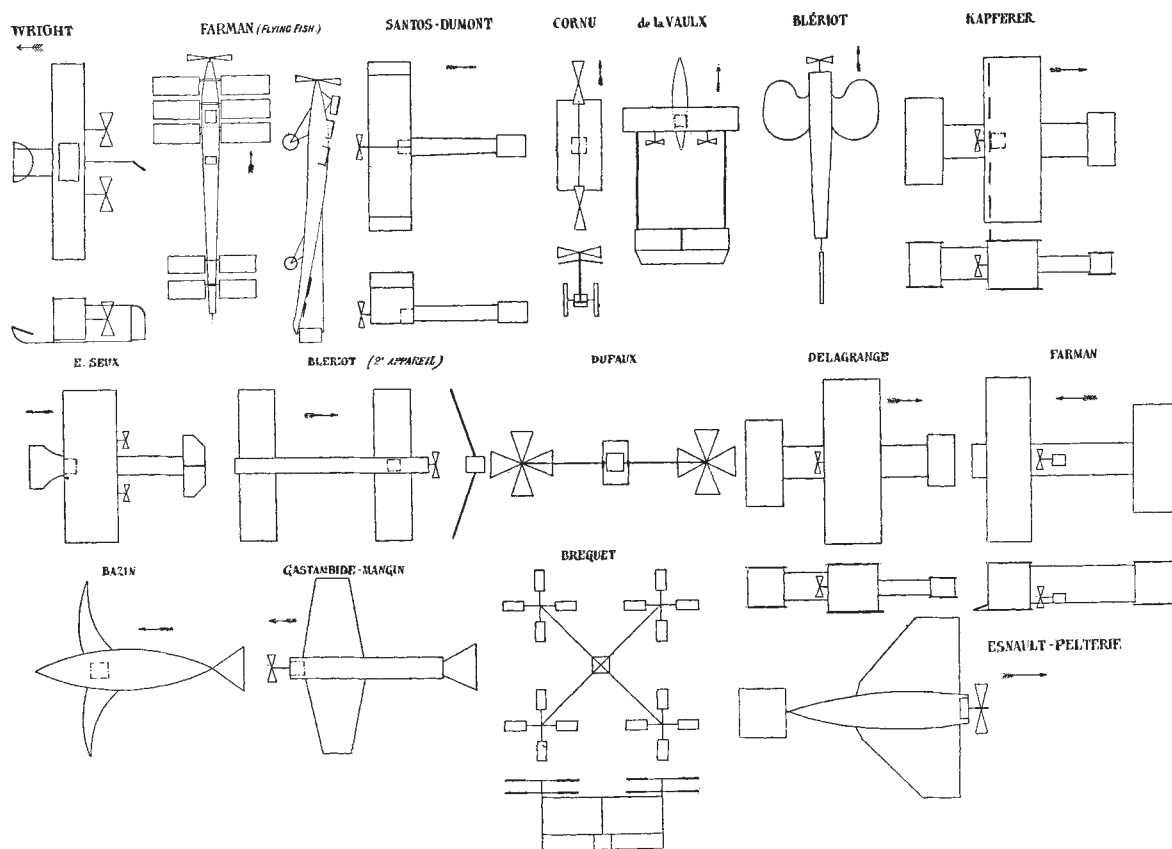
[1] "Airship Propeller Problems," Trans. Acad. Sci., St. Louis, xvii., No. 1.
[2] *Aëronautical Journal*, April, 1908.

longitudinal stability of the aërostat or aëroplane, depends, we believe, on the roots of a biquadratic equation, but in this case there appear to be two critical velocities, one an inferior and the other a superior limit to the speed. Of these the superior limit has been reached in bicycling in the wake of an express train. But bicycles are frequently ridden at speeds below the inferior limit, being kept upright by careful balancing involving no *conscious* effort on the part of the rider. It is highly probable that in many circumstances longitudinal instability may be equally well counteracted by the unconscious efforts of the aviator. Regarding the recent successes, evidence is far too conflicting to enable judgment to be passed in this review as to whether the machines were really stable, though there is equally no evidence to show that they were not.

" Now M. Léon Delagrange, after making quite a number of short flights (the longest about 200 feet) with his motor flying-machine, has found it advisable to go with M. Voisin, the cleverest of the French flying-machine pilots, to experiment with a gliding machine on the sand-hills near Le Touquet." [1]

However probable it may be that a man-carrying machine is automatically stable, the performance of a successful directed flight can never definitely answer the question how far the success is due to automatic stability and how far to the skill of the operator. It may be that with a little experience something short of truly automatic stability is sufficient for all practical purposes; on the other hand, a great many writers who place their views before the public insist on automatic stability as a *sine quâ non*. The evidence derived from uncontrolled aërodromes such as those

WRIGHT　FARMAN *(FLYING FISH)*　SANTOS-DUMONT　CORNU　de la VAULX　BLÉRIOT　KAPFERER

E. SEUX　BLÉRIOT *(2e APPAREIL)*　DUFAUX　DELAGRANGE　FARMAN

BAZIN　GASTAMBIDE-MANGIN　BREGUET　ESNAULT-PELTERIE

Plans of the principal Aëroplanes. From " Le Problème de l'Aviation," with slight modification.

Chanute long ago experimented on automatic stability, and stated that his gliding machines had special appliances for securing it.[1] It is scarcely possible that Chanute's methods have not been utilised by the Wrights. Yet according to the papers the French aviators, while expressing great admiration for the Wright performances, are of opinion that the successful balancing of the Wright machine is mainly a feat of skill on the part of the aviator, and that their object has been to construct machines with which anyone can fly. In support of this view we read that " neither M. Delagrange nor Mr. Farman had ever driven an aëroplane before the last eight months." [2] On the other hand, we were told more than a year ago that

used by Langley enables the question of automatic stability to be tested much more definitely. The recent reprint of Langley's researches will always prove a valuable contribution to the literature of aviation.[2] But in employing results of experiments with small models to draw conclusions about larger machines, everything depends on a correct appreciation of the theory of dimensions, and who is there that is sufficient of a mathematician that he can be absolutely trusted not to drop into one of the innumerable pitfalls that beset this elusive but valuable method of generalisation?

Even to make a machine fly steadily in a horizontal

1 See, *e.g.*, *Cassier's Magazine*, June, 1901.
2 *Aëronautics*, August, 1908, p. 61.

1 *American Magazine of Aëronautics*, July, 1907, p. 8.
2 "Researches and Experiments in Aërial Navigation." By S. P. Langley. Reprinted from the Smithsonian Reports. (Washington: Government Printing Office, 1908.)

line at all, three conditions are required. It is not sufficient that the drift should equal the thrust of the propeller (supposed horizontal), and that the lift should equal the weight. There is the third condition that the three forces, weight, propeller thrust, and result-ant air resistance, must pass through one point, or an equivalent condition obtained by equating moments. If this condition is satisfied, but not otherwise, the machine is properly *balanced,* and may fly straight. But its flight is not necessarily *stable,* and it may upset at any moment. To find if it is longitudinally stable we must examine what happens if it deviates from its course and begins to pitch. To specify its motion at any instant in this case *three* variables are required, as every student of elementary mechanics ought to know. The resultant air resistance will also be altered, and to specify the new resultant three other variables will be required. The connection between these and the preceding three depends on the laws of aërial resistance. This connection is specified by certain " coefficients of stability " the values of which are necessarily based on experimental knowledge. On the assumption that if these are known, and the weight, position of the centre of gravity and moment of inertia of the flying-machine are known, the oscillations have been worked out and the condition of stability de-termined. This condition is conveniently expres-sible in terms of a critical velocity, it being neces-sary for stability that the velocity of a machine flying in a given manner should not be less than the corre-sponding critical velocity given by theory. In the case of a balloon we have learnt, on the other hand, that the velocity must not be greater than the critical velocity. The existence of a critical velocity was re-cently pointed out by Mr. Lanchester in a communi-cation to the British Association, and it is to be hoped that his remarks will carry some weight with the pre-eminently unpractical " practical men " who abound in this country.

When these results were obtained it still remained to reduce the problem of stability to the form of rules which were not beyond the ken of the ordinary work-ing mechanic, and, further, to show how the necessary data could be obtained from experiments on models. Had the present writer been able to give his whole time to this work the problem of stability would have been thrashed out to the bitter end long ago. Looking at the matter perfectly impartially, and in view of many cases of a similar kind that may occur in almost any branch of science, the question may be asked whether it is desirable that the completion of such investigations should be delayed indefinitely because those who are prepared to undertake them are de-barred by their professional duties from giving the necessary time? The cost of a mathematician's time in working out such a problem would probably not exceed the cost of building a single flying-machine, so that the existing method of trial and error is certainly not to be recommended on the ground of *cheapness.*

The critical velocity of a machine moving in air depends on the position of its centre of gravity, the moment of inertia of the machine, the form, dimen-sions, and position of its supporting surfaces and tail, and the position of its propeller. In some cases stability may be increased by increasing the moment of inertia; in other cases it may be decreased. Our work tended to show that a machine might become unstable if the moment of inertia were *either* too large or too small, other things being kept constant. But when the mathematical theory has been worked out in every detail, the coefficients of stability for any given machine must necessarily depend on experi-mental data. Now the average mechanic understands the importance of finding the resultant thrust on an

aëroplane, but he does not realise the necessity of finding the centre of pressure through which this thrust acts. The result is that experimental data are far from complete on the very points in which they are most wanted. If, however, we were to try and base our stability calculations entirely on the experi-mental data obtained for the separate aëroplanes, we should not only have a good deal of calculation to perform, but at the end we should have omitted to take account of the resistance of the framework, car, and rider. A simpler plan would be to construct a stabilimeter [1] for experimenting on models as a whole instead of with single aëroplanes. When a machine begins to pitch and rock it has a rotatory as well as a translatory motion, and the rotation may, and cer-tainly does, influence the magnitude and position of the resultant thrust of the air. No calculation of stability can be considered valid which does not take account of this influence. One might just as well neglect the wedges of immersion and emersion in working out the stability of ships. On this turning effect, as it might be called, we have no experimental data whatever. But if a model is to be tested in a stabilimeter, the mechanic will require simple work-ing rules for applying his results, and these must in the end be laid down by mathematicians. In parti-cular he will have to be told whether he can improve the stability of his model by altering the positions of his aëroplanes or the moment of inertia of his machine. A number of questions require answering, and the answers require putting in a simple form. Here is one example: In a dirigible the critical velocity represents the greatest velocity consistent with stability; in an aëroplane system it represents the least velocity. If, starting with a dirigible, we add aëroplanes and reduce the size of the balloon gradu-ally down to nothing, we must come across an inter-mediate type which is either always stable or always unstable. What is this type?

The recent flights show what can be done in aviation by a person possessed of skill and experience. They are a necessary factor in the development of artificial flight. The problem is quite in a different position from what it was a year ago. But if flying-machines are to be made accessible to the million, the sooner English aëronauts learn mathematics or get someone to do the mathematics the better. At the present time a great deal of rubbish passes off as mathematics which is quite unworthy of the name. We may instance the use of Taylor's expansion in infinite series to prove, not even that the reciprocals of a harmonical progression form an arithmetical pro-gression, but that the general term of this arith-metical progression is of the form written down in elementary text-books on algebra.[2] Or, again, the discussion of the details of an example which would be in a more proper place in a school text-book or examination paper on elementary trigonometry.[3]

Mr. Lanchester's book, of which the first volume has been noticed in NATURE and the second will be reviewed shortly, should open the eyes of many would-be aëronauts as to the complex theoretical investiga-tions which have to be mastered in any attempt to reduce the problem of flight to an exact science. Although the author has purposely avoided, so far as possible, the use of mathematical formulæ, the reader who aspires to revolutionising the flight problem with-out making actual experiments and without an ex-tended study of mathematical or physical principles will find the book a pretty hard nut to crack.

The time has, however, passed when any useful

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[1] *Cornhill Magazine,* May, 1907.
[2] *Aëronautical Journal,* April, 1908, p. 27.
[3] *Aëronautical Journal,* January, 1904, pp. 4, 5.

purpose can be served by merely writing to the effect that the proper way of solving the problem of flight is by means of vertical screws or by imitating the action of birds' wings. When people can fly for an hour by one method they will scarcely be likely to try another. An actual demonstration of either of these alternative methods as applied to a man-carrying machine would, of course, be watched with considerable interest. Whatever may be the best and cheapest way of advancing aëronautical knowledge, it is probable that the human element and the feeling of " every man his own flying-machine " will appeal most to the Englishman, and more scientific methods will appeal more to the German, who has already arranged for translations of Mr. Lanchester's works.

Mr. Herbert Chatley [1] has directed attention to the part played by eddy formation in determining the flow of air in the wake of aëroplanes. This factor may introduce dangers in a flying-machine should the rate of eddy formation coincide with the period of free oscillation. Accidents from a similar cause have frequently occurred in other branches of engineering, and it seems very probable that some day we shall have an object-lesson of the kind in aëronautics. But the study of these eddies affords an interesting re-creation for those who like to look into the matter. The side of a ship is a good place for watching eddy formation, but a better place is a dusty road along which motor-cars are passing. Here anyone can see the eddies being thrown off at perfectly regular intervals, each picking up a separate cloud of dust and whirling it high into the air. If the observer cared to carry his researches further he might get a motor-car, and try attaching tails of different sizes and shapes to it until he got one in which the eddy formation was reduced to the smallest possible amount, and the air resistance would probably be also reduced. He would not succeed in abolishing dust altogether, nor would he make a fortune by taking out a patent; but he would discover a more effectual means of reducing the dust nuisance than by writing complaints to the newspapers. G. H. BRYAN.

MARINE BIOLOGY.

THE work of the Danish naturalists on behalf of the International Commission for the Investigation of the Sea is greatly enriching our knowledge of the natural history of important sea-fishes. In " Serie Fiskeri," Bind ii., Nos. 5–8, of the Danish " Meddelelser fra Kommissionen for Havundersø-gelser," there are several important papers by Dr. Johs. Schmidt, a naturalist well known for his discovery of the breeding-places of the common eel of European rivers.

In No. 6 Dr. Schmidt records the results of marking experiments with plaice and cod in the waters around Iceland. Of numerous mature plaice caught, labelled, and liberated in the summer of 1905 off the north and east coasts respectively, those re-captured of the former batch were found to have travelled westward, the latter southwards along the east coast and then westwards along the south coast, *taking in each case the shortest route to the warm Atlantic waters*. Here in winter and spring they spawn. The eggs and fry are then carried passively along by the Atlantic stream (Irminger current), which sets eastwards in spring and summer, and the just-transformed young appear successively in the bays and fjords, first on the west, later on the north, and later still on the east coast. Immature cod, caught, labelled, and liberated at the same time as the plaice on the north-east coast, did not migrate, but remained

on that coast throughout winter, and even for one to two years after liberation. It was also found that one-year-old cod were much more numerous on the east coast than on the south and west. On the other hand, the eggs of the cod was absent in hauls with pelagic nets made on the north and east coasts, but plentiful on the west and south. From these facts it appears that the peculiar hydrographical conditions round Iceland involve a double migration of considerable extent on the part of cod and plaice, viz. (1) the passive drift with the eastward-setting Atlantic stream of eggs and fry born off the south and west of the island; (2) the active return migration of spawning fishes to the warm water, due to their special sensitiveness to external conditions on the approach of the spawning season.

Another noteworthy result of these marking experiments is to show that Iceland plaice grow at an average annual rate of 2–3 centimetres, much slower, therefore, than in the North Sea. The most obvious cause of this is the low temperature of the water, on the east and north coasts especially, and the short summer. It is also interesting, from the practical fishery standpoint, to note that more than 60 per cent. of the re-captured marked plaice were taken by English steam trawlers.

Hitherto the post-larval stages of those North Atlantic gadoids, viz. the hake (*Merluccius vulgaris*), *Molva elongata*, and *Raniceps raninus,* have not been described or figured in literature. In No. 7 Dr. Schmidt gives descriptions and figures of thirteen different post-larval stages of the hake. All these were carefully identified with the adult by counting the numbers of vertebræ and fin-rays. The paper also contains a summary of the distinctive characters of various gadoids in the post-larval stage.

In No. 8 there are described seven different post-larval stages of *Raniceps raninus,* a remarkable-looking plump form, somewhat resembling a post-larval Liparis species, and differing from other post-larval gadoids in not possessing the usual three post-anal bars of pigment. In this number also are described and figured four different post-larval stages of *Molva elongata* and *Molva byrkelange*. These two forms, in regard to which Holt and Byrne thought " a single species was enough for the reception of both," are shown to have perfectly distinct post-larval forms with characteristically different arrangements of pigment. The geographical distribution of the two forms, as Dr. Schmidt points out, is also quite different, so that there can now be no doubt that we have to deal with two species.

In No. 5 Ove Paulsen describes and figures all the species of Peridiniales so far known to occur in Danish waters. The Peridiniales are a most important group of unicellular organisms from the standpoint of the student of plankton, since certain species of them appear to be characteristic of water of neritic and oceanic origin respectively. The descriptions appear to be adequate, and there are usually figured several different views of each form. There is also a copious list of literature at the end.

NOTES.

At the general meeting of the Royal Society of Edinburgh, held on October 26, Sir William Turner, K.C.B., F.R.S., was elected president of the society.

THE King has granted his Royal licence and authority to Dr. Ludwig Mond, F.R.S., to wear the decoration of the Grand Cordon of the Crown of Italy conferred upon him.